BEBR
FACULTY WORKING PAPER NO. 90-1669

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August 1990

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Abstract

In this paper, I suggest that strategy research should concern itself with continuing the conversation of the field rather than insisting upon a place for universal methodological criteria within that conversation. I attempt to sustain the dialogue begun by Bourgeois, Bowman, Jemison, Huff, and others, who recommend the pragmatic approach of methodological and theoretical pluralism as the best way forward in increasing empirical content. I draw heavily on the philosophical writings of Dewey, Kaplan, and Rorty and the methodological essays of economists such as Boland, Caldwell, and McCloskey in my effort to persuade others in the strategy field that "good science is good conversation".

STRATEGIC MANAGEMENT AND DETERMINISM: SUSTAINING THE CONVERSATION

... once or twice she had peeped into the book her sister was reading, but it had no pictures or conversation in it, 'and what is the use of a book,' thought Alice, 'without pictures or conversation?'

-- Lewis Carroll (1865, p.5)

INTRODUCTION

Bourgeois persuasively argues that: "reductionism eliminates much of the richness that characterizes the strategic management process ..." (1984, p.586). Similarly, Bowman (1990) suggests that reductionism is an ever-present risk, where one pushes roughshod over issues and constructs toward a central paradigm. In contrast, Teece (1986) argues that strategy requires a dominant research program and Camerer offers a "manifesto" (1985, p.1) for rigorous, deductive policy research. While I agree with Camerer that organizational economics and industrial organization are worthwhile pursuits for strategy research (Mahoney, Tang, & Thomas, 1990), I believe that the theoretical and methodological pluralism advocated by Bourgeois, Bowman and others (Boland, 1982; Caldwell, 1982; Denzin, 1989; Huff, 1981; Jemison, 1981; Jick, 1979; McCloskey, 1985) is the more persuasive argument.

I want to emphasize that disagreement does not entail disrespect. The main philosophical point, made by Plato and other followers since, is that any criticism is better than a dismissal or an oversight. Montgomery, Wernerfelt and Balakrishnan (M-W-B) call for a "more active and public dialogue, including published comments, rejoinders and criticisms" (1989, p.194). Similarly, Bowman (1990) submits that intellectual exchange or arguments are quite useful and not as common yet in our field as they should be. I am persuaded that good science is good conversation (McCloskey, 1985; Rorty, 1979), and wish to continue the dialogue begun by Bourgeois, Bowman, Camerer, M-W-B and others.

In the first section I consider Camerer's proposal of a rather narrow perspective for the strategic management field involving the use of deductive paradigms, mathematics, and economics as the main research tools. I express my concern that a restriction of the field to analytically tractable questions would be counterproductive to the future growth of strategy research.

In the second section, I consider M-W-B's methodological prescriptions and proposals. I suggest that M-W-B hold a view between the pragmatist camp (Bourgeois, 1984; Bowman, 1990; Dewey, 1929; Rorty, 1979) and the logical positivist camp (Blaug, 1980; Camerer, 1985; Popper, 1934). I hope to persuade M-W-B, and many others in the mid-range, to consider the positive consequences of pragmatism and pluralism.

REDIRECTING RESEARCH IN STRATEGIC MANAGEMENT: A REPLY TO CAMERER

Camerer's major criticisms of the strategic management field include the following:

1. The field is plagued by confusion about its basic concepts (p. 2).

2. The field has failed to test its theories and models properly (pp. 2-4).

3. It is not clear whether the field is an art or a science (pp. 4-5).

4. The field has placed an excessive emphasis on induction over deduction as a method of scientific inquiry (pp. 5-7).

1. <u>Confusion about basic concepts</u>. Camerer notes that there is no clear definition of strategy (Leontiades, 1982) and that the exercise is futile with the "awkward grammar of English" (p.2). Or put differently, the dictionary is a book of circular reasoning. What do we mean by the word "mean"? What do we mean by the word "word"? If not for the fact that some students would take Camerer's objection seriously, the argument would only be funny. Camerer fails to recognize the vagueness inherent in all concepts. The concept of "pure" elements is a mixture of isotopes, the concept of "absolute" temperature is measured from an approximate zero. How much more precise are economists when they discuss an "industry" or a "strategic group" or an "innovation" or psychologists when they talk about "intelligence" as they "mismeasure man"? (Gould, 1981).

The point is that concepts are indefinitely indefinite. This concept of concepts does not imply that our thinking should be fuzzy to achieve an accurate representation of a fuzzy world. What I am suggesting is that it is possible to take a more positive view of our conceptual fuzziness than Camerer appears inclined to. While one may agree with Camerer that vague concepts are "a symptom of disease" (1985, p.2), this does not thereby diagnose a failing.

Furthermore, the demand for exactness of conceptual meaning may have a pernicious effect. The result may be a premature closing of the mind. After all, the concepts in terms of which we pose our scientific questions limit the range of admissible answers. That members of the strategic management community view the concept of strategy differently is an essential tension for healthy creativity. In fact, one can make the case for allowing multiple conceptualizations of strategy to flourish, as long as there exists an acceptable level of correspondence between the theoretical concepts/constructs and their operationalizations/ measurements in empirical studies (Chaffee, 1985; Frederickson, 1984).

As a reply to Camerer, I have been persuaded that strategy is a "continuing search for rent" (Bowman, 1974, p. 74) and the protection of these Ricardian rents via human, physical, locational, organizational, and legal capital (Kogut, 1984; Rumelt, 1984; Teece, 1982; Wernerfelt, 1984). I would be more sympathetic to the argument that my view is too narrow (Chaffee, 1985) rather than the contention that I am not being precise.

2. <u>Failure to Test Models Properly</u>. Regarding Camerer's second major assertion, that the field has failed to test its theories and models properly, his sole criterion of a "proper test" appears to be predictive ability, although he notes that it "is not the **only** test of a good theory" (1985, p.4, emphasis in original). Camerer articulates the philosophical view of **instrumentalism**, claiming that theories are best viewed as nothing more than instruments. Scientific models are "inference tickets" for making predictions. Camerer (1985, p. 3) argues that: "Predictive ability should be the fundamental test of a theory, or at least a 'mature' theory (Blaug, 1980), often at the expense of surface realism or truth of assumptions (Friedman, 195(3))".

Several arguments have been made against instrumentalism. I will discuss four arguments here (the first two are addressed by Camerer). While the first three arguments against instrumentalism I find unpersuasive, the fourth argument against instrumentalism I believe is compelling:

A) The emerging field of strategic management should not be subjected to the empirical scrutiny appropriate for a mature discipline.

B) Prediction does not guarantee understanding.

C) Why should we take a model seriously when the author uses egregiously false assumptions?

D) Prediction (falsification) is impossible.

Camerer dismisses the first argument as a self-serving protectionist stance. I concur. I believe that the strategy field is virile enough to avoid the tendency to immunize theories against criticism. Strategy research does not need a defensive methodology (Blaug, 1980).

Camerer's second argument raises doubts about the symmetry thesis in the writings of Hempel (1966). The symmetry thesis is that explanation is simply "prediction written backwards". Camerer notes that this symmetry thesis is incorrect. He submits that: "prediction does not **guarantee** understanding". I concur. It is only too obvious that prediction does not guarantee explanation. Students crank out countless tables of highly significant t-statistics from garbage-can regressions, where the only discernible rationale appears to be the maximization of adjusted R-square.

I would also point out that explanation does not guarantee prediction. Darwinian theory is a standard example. Thus, prediction need not imply explanation and explanation need not provide prediction. One may attack Camerer's instrumentalist view by arguing that strategic management ought to do better than merely predict accurately. Nagel (1961, p.4) argues that: "it is the organization and classification of knowledge on the basis of explanatory principles that is the distinctive goal of the sciences". Popper rejects instrumentalism as untenable because it does not urge scientists to practice a critical methodology; it is satisfied with high correlation and does not push the scientist to consider fuller explanation (1965, p.103).

Camerer, while seeming to acknowledge this point, does not back down from his instrumentalist stance. I submit that there is no inconsistency here since Camerer's acceptance of Friedman's

(1953) "as-if" formulation positively rules out the possibility of causal explanation. In short, instrumentalism is its own defense, and perhaps its only defense (Boland, 1979, p.522). Toulmin (1972) provides an eloquent defense of instrumentalism against the attacks of Nagel and Popper. There are two other major objections that Camerer did not consider in his paper.

The third criticism against instrumentalism is that models with false assumptions may automatically be dismissed. This is the philosophical view of the **ultra-empiricist** (Blaug, 1980) who insists on testing each assumption of the model. I emphatically take issue with this stance and on this score remain in Camerer's camp. To reject a paper on the sole ground that its assumptions cannot be tested is uninformed.

In fact, to reject a paper on the sole basis that its assumptions are false is unwarranted. To make the statement that "the author's false assumptions thus lead to false conclusions" is a non sequitur. We may argue in favor of the conclusion from the truth of the assumptions (modus ponens) or we may argue against the truth of an assumption by the falsity of a conclusion (modus tollens). However, truth cannot be passed backwards and **falsity cannot be passed forwards.**

That a model does not have a one-to-one correspondence with the world is why we call it **theory.** Metaphorically speaking, a theory may be thought of as a road map in which a larger more 'realistic' map may be of less guidance than a smaller ('unrealistic') map. In fact, a map of Illinois that was the exact size and shape of Illinois would be very realistic but of no utility in helping us find our way. To apply this metaphor to economics, the assumption that human beings have perfect rationality may be an 'unrealistic' premise but does not damn the whole economic literature (Friedman, 1953). Counter-intuitive and apparently refuted assumptions may lead to useful conclusions.

Some in strategy insist that researchers use true assumptions, while on the other hand an overwhelming majority of economists mandate that researchers use false (perfect rationality) assumptions. I find the former view uninformed and the latter stance excessively dogmatic. I suggest that papers that assume perfect rationality, bounded rationality, enacted rationality or come up with an idiosyncratic X-rationality may each provide insight.

To summarize, I have found the first three arguments against Camerer's instrumentalism unpersuasive. However, the fourth argument, which Camerer fails to address, I find quite damaging: Prediction (falsification) is impossible in economics and strategic management.

The industry of making predictions, including economists and strategic management researchers, earns merely normal returns (McCloskey, 1985). Camerer alludes to the "specification problem" (1985, p. 4) of hypotheses that are indistinguishable given the available data. I want to argue here that the real insights come from "intuition" and "grounded theory" (Glaser and Strauss, 1967) rather than deductive reasoning and empirical testing.

Consider the following example: I am a researcher in education and am concerned with improving the French-test scores of students in an elementary school (grades 1-8). Being a freshly minted Ph.D. student indoctrinated in the reductionist tradition I am espousing my theory that: French test scores = F (Weight). I run my regression and find that the weight variable is positive and significant. I make a policy recommendation that the students should have two double malteds each day to improve their test scores. One of my colleagues suggests that I may have left out a significant variable, that being the age of the child. I rerun my regression: French test scores= F(Weight, Age) and find that the weight variable is negative and significant. I reverse my policy recommendation and suggest that students be given salads at lunch to reduce their weight and thereby improve their test scores.

Another colleague points out to me that the boys in the school tend to take math, while the girls (who weigh relatively less than the boys of the same age) tend to take languages and that this may explain the negative coefficient that I obtained. I rerun the regression: French test scores = F (Weight, Age, DUMMY). The DUMMY variable is set to 1 if the student has taken French and is set to 0 otherwise. Now the weight variable is insignificant. Age and whether the person has taken French lessons explain much more of the variance in French test scores than does the weight variable (for more sophisticated examples of the "identification problem" from a professional econometrician, see Leamer, 1983).

Now in this model we can follow by intuition the problems of mispecification error and yet with phenomena we know much less about we put great confidence in our estimates of a variables' significance. Nelson Goodman notes that: "Truth, far from being a solemn and severe master, is a docile and obedient servant. The scientist who supposes that he is single-mindedly dedicated to the search for truth (via falsifiability) deceives himself ... He as much decrees as discovers the laws he sets forth, as much designs as discerns the patterns he delineates" (1978, p. 18). Kuhn noted that: "the scientist often seems rather to be struggling with facts, trying to force them into conformity with a theory he does not doubt" (1977, p.193). In practice there is not much falsifying going on.

The fact of the matter is that the scientific community has Bayesian a priori beliefs that impact on what empirical results they will accept. Thus, for a time a community of scholars might reject "solid" results, for instance that transaction costs impact on the vertical integration decision, and accept "feeble" ones, for instance that market share leads to higher profitability. The beliefs can be reversed without changing the example.

There is another problem with the falsifiability thesis that Camerer advocates, namely, that it fails to recognize that **all facts are theory laden and all theories are value laden.** No guillotine humanly devised can sharply split facts and values. The contents of observation itself cannot be free from conceptual contamination. A scientific theory is a system of concepts, hypotheses and observations that are inextricably intertwined.

The denial of objectivity in social science is more common in sociology than in economics. However, one Noble prize winner in economics, Gunnar Myrdal, went against the stream in arguing for the concept of value-impregnated social science. Myrdal's (1970) solution is not to suppress value judgments but rather to state them at the outset. Pretending to separate normative and positive statements is self-deception. To argue that good scientists practice the objective criterion of falsification is pure fiction.

Camerer's insensitivity to values that are implicit within theories is evident when he states that: "the concept of equilibrium, (is) a state in which everyone is happy and nobody can improve their lot" (1985, p.7). In point of fact, an optimal, efficient equilibrium may exist in which one person has virtually all the wealth and everyone else is at the subsistence level. I would hardly say that "everyone is happy" by this dismal outcome. An efficient market equilibrium is also a mechanism of denial. There is no happiness or justice (Rawlsian or otherwise) implied by equilibrium.

I would argue that the claim that knowledge can be free from doubt, metaphysics, morals, and personal convictions is self-deception of the most hurtful kind. Scientific knowledge is no different from personal knowledge (Polanyi, 1962). Trying to make it different by setting up arbitrary standards (and the criterion of falsificationism is as ad hoc as any) "if rigorously practiced would lead us all of voluntary imbecility" (1962, p.88).

If one finds this sweeping argument against the falsifiability thesis unpersuasive, then consider the following arguments: First, chance is the ever-present alternative that spoils falsification. Second, the possibility is always present that the experiment was not properly controlled. Third, as my French test model illustrated, there is the identification problem. Fourth, tests of models are not tests of theories. Fifth, the French physicist Duhem (1906) and the philosopher Quine (1953) have correctly pointed out that an experiment can never condemn an isolated hypothesis.

Suppose that the hypothesis H (that related diversification leads to higher profitability in May, 1990) implies a testing observation O (an increase in the Palepu measure of related diversification indicates that ROI increases). Only the addition of ancillary hypotheses H1, H2, H3 and so forth makes measurement possible (H1 = the theory applies to the United States for 1981-1989; H2= industry effects do not confound the results; H3= interactions of importance have been controlled for; and so forth). Thus, not-O implies not H -- or not H1 or not H2 or not H3 or any number of failures of premises irrelevant to the main hypothesis in question.

Popper himself has endorsed Duhem's irrefutability thesis (1965, p. 50). It takes many premises to reach a conclusion. When a conclusion is shown to be false it is impossible to pin down the hypothesis that is the culprit. An experiment can never condemn an isolated hypothesis but only a whole theoretical group. Ancillary hypotheses insulate a hypothesis from a crucial test. Falsification, near enough, has been falsified (McCloskey, 1985).

As a voice of pragmatism, I would argue that usefulness is as valid a judge of a framework's cogency as its predictive power. The validity of a framework depends upon the consequences of acting upon it. Alice, as she was falling down the rabbit's hole asked "do cats eat bats?" or do "bats eat cats?" (Carroll, 1865, p.9). She noted that she couldn't answer either question and thus, it didn't much matter which way she put it. Alice was a pragmatist. There is no difference, that makes a difference between, "it works because it's true", and "it's true because it works" (Rorty, 1982). Rules of rationality derived from deductive models that are not backed by executable algorithms are a worthless currency (Simon, 1982). Or put differently, they don't work.

While methodological falsificationism is a noble quest, it would be a tragic mistake if its name were invoked as an incantation for rejecting other social scientists' works. Camerer suggests that: "Most models or frameworks in policy research, if tried before the stern judge of predictiveness and her sterner cousin, relative predictiveness, would be convicted and be sentenced to perish rather than be published" (1985, p.3). Why do strategic management researchers have to defend their framework by this ad hoc criterion, and before what tribunal? I ask along with McCloskey: "Why do we need methodological rules to govern me and thee enforced by an intellectual hue and cry in which we will stone thee if thou resisteth and expel thee from the tribe?" (1985, p.23). A methodological authoritarian is a Red Queen: you broke the methodological rules, "off with your head".

The point I want to emphasize is that hard working, honest and sensitive scholars make methodology great rather than adherence to methodological rules making scholars great. Strategic management, like any "mature" field, should get its standards of argument from itself.

The search for the absolute paradigm is the search for absolute conformism. Any method that encourages uniformity is a method of deception. "It enforces an unenlightened conformism and speaks of truth; it results in the deterioration of intellectual capabilities, of the power of imagination and speaks of deep insight" (Feyerabend, 1975, p. 45). The price of such methodological training would be a trained incapacity.

I fundamentally challenge the notion that a static set of procedural rules for the appraisal of theories or for the definition of appropriate theoretical structure has ever been, or should ever be, followed by researchers in their attempts to gain knowledge. Persuaded by Rorty (1979, 1982, 1987), I openly question the whole epistemological exercise. The preoccupation with methodological standards is self-defeating and the motives of the exercise may be viewed primarily as an attempt to eternalize a certain contemporary language-game, social practice or self-image (Rorty, 1979).

I hold then that a rules-oriented methodology is self-deceptive and objectionable. The desire for methodological rules is a desire for certainty and is a cowardly "escape from freedom" (Fromm, 1941). I believe that Bourgeois, following Dewey (1929), is correct when he states that: "the ubiquitous quest for the reduction of uncertainty" (1984, p. 587) is driven by the need for psychological security. I also believe that Bourgeois is correct in noting that the pursuit of deterministic solutions forces reductionism. However, once this idea has become conscious, we may come to realize that the sense of closure (while satisfying because it is preceded by the tension of perplexity) is self-deception.

A methodological authoritarian, while perhaps having the intentions of a benevolent dictator, blocks the path of inquiry. I have little doubt that Kaplan said it better: "The conflict between freedom and control is an existential dilemma for science, whatever it may be for society at large. Yet for science, at any rate, it seems to me that reason requires that we push always for freedom, freedom even for the thought that we enlightened ones so clearly see to be mistaken" (1964, p. 377).

Over a hundred years ago the economist Jevons suggested that mutiny in the field of social science would increase the nation's bounty: "In matters of philosophy and science authority has ever been the great opponent of truth. A despotic calm is usually the triumph of error. In the republic of the sciences sedition and even anarchy are beneficial in the long-run to the greatest happiness of the greatest number" (Jevons, 1871, pp. 275-276).

While the words sound threatening, I emphasize that the ideas expressed should not be considered menacing to the post-Kuhnian scholar. In fact, I believe that moving away from a rules-based "reductionist" methodology is consistent with an open, plural, and pragmatic community. Greater epistemological appreciation provides a basis for methodological understanding and **tolerance** of diversity and multiciplicity in research design (Evered and Louis, 1981). Why should anyone feel threatened by tolerance and understanding?

Strategic management scholars need not conform to a central paradigm nor decree inflexible methodological principles; on the contrary an ungroundable but vital sense of human solidarity in our intellectual community may develop and deepen by the acknowledgement and acceptance of the right to differ (Rorty, 1982). Rorty (1987) has persuaded me that a group of scholars may respect the contingencies of language, of selfhood, and of a liberal community.

3. <u>Strategy: Art or Science?</u> Camerer seems caught up with the demarcation criterion of determining science from non-science. My position is that the question is moot. One may read endless debates on whether economics is a science in the economics journals from the late 1890's to the present. If I make any contribution to the field, I would urge that we not divert scarce resources to this demarcation problem in strategy. I suggest that strategic management is important and I am profoundly indifferent to the science vs. non-science "war of the words". In fact, my position

is that there is no meaningful way to separate science from non-science, so that the demarcation problem posed by Camerer, and which is so important to an economist such as Blaug (1980), is a pseudo-problem. The demarcation problem serves chiefly to demarcate "us from them" and appeals to those that desire a nobler self-image.

While I share Camerer's enthusiasm for the use of decision theory, game theory and industrial organization, I would also argue for the inclusion of psychology, organization theory, sociobiology, sociology, history and institutional analysis (Bowman, 1990). The strategic management scholar should not discriminate on the basis of race, creed, or epistemological origin. The interaction of individuals, possessing different knowledge and different views is what constitutes the life of thought and the lifeblood of advances in strategy formulation.

Camerer argues that: "Unfortunately, policy approaches do not seem to pass these tests of time; knowledge in policy analysis is neither timeless nor cumulative" (1985, p.4). First, I suggest that the search for timeless knowledge is the Cartesian quest for timeless certainty over the quest for wisdom (Dewey, 1929). The noted economist Sir John Hicks argued that (1976, p. 208):

> Since it is a changing world that we are studying, a theory which illumines the right things now may illumine the wrong things another time. This may happen because of changes in the world (the things neglected may have grown relative to the things considered) or because of changes in the source of information (the sorts of facts that are readily accessible to us may have changed) or because of changes in ourselves (the things in which we are most interested may have changed). There is, there can be, no economic theory which will do for us everything we want all the time.

I conclude that strategic management cannot be timeless, which is the special reason why strategy is prone to paradigm shifts. Universality is qualified by specificity, immutable verities are challenged by recognition of changing patterns of investigation and patterns of thought; logical analysis is checked by the study of history.

Second, in the post-Kuhnian age I question whether knowledge is cumulative. Indeed, Kuhn's thesis is that the textbooks of science which tell a story of how the field has progressed by a cumulative process is pure fiction. Kuhn's (1970) classic on the nature and significance of scientific revolutions is much cited and little read. Feyerabend (1975) develops further the thesis that the claim that new theories incorporate older theories is largely a myth.

Camerer (1985, p.1) also discusses metaphor in a pejorative manner. Camerer fails to appreciate that most of our convictions in life, let alone strategic management, are driven by metaphor (Morgan, 1986). In fact metaphors are central to our epistemological beliefs (such as the Lockean notion of the "mind as a mirror"). Rorty maintains that: "It is pictures rather than propositions, metaphors rather than statements which determine most of our philosophical convictions" (1979, p.12).

In strategy, an inevitable debate is emerging on whether we follow the metaphor of equilibrium derived from physics or the metaphor of Darwinian evolution (Alchian, 1950) as the proper image for understanding our "institutions of capitalism" (Williamson, 1985). It should also be noted that models are metaphors, and "game theory" (which Camerer espouses) by its very name suggests a metaphor. A game theorist may begin his or her seminar by suggesting to the audience that they "consider the strategic interactions of Folgers and Maxwell House as a two-person, zero-sum 'game'". The real world interaction between firms is said to be "like" a game-theoretic model. In agency theory a firm is "like" a nexus of contracts.

Black has pointed out that: "a memorable metaphor has the power to bring two separate domains into cognitive and emotional relation by using language directly appropriate to one as a lens for seeing the other" (1962, p. 236). The economist McCloskey suggests that: "Perhaps thinking is metaphorical. Perhaps to remove metaphor is to remove thought" (1983, p. 503).

Perhaps Camerer views metaphorical argument as sophistry and that mere persuasion is unpersuasive. According to Camerer, we must be more "rigorous". It should be noted that many social scientists become quite defensive when it is suggested that their discipline is based on consensus rather than the hard, objective truth of the natural sciences. The usual response is to demonstrate how the field has enforced stringent methodological principles to be more "objective". This defensive attitude is due, in part, to the premise that the natural sciences are not based on "mere" consensus.

However, even in the field of mathematics where the Cartesian quest for the indubitable is said to reach its fulfillment, some highly respected mathematicians have suggested that their field is also determined by consensus (Davis and Hersh, 1981). Kline (1980, p. 6) notes that there is no rigorous definition of rigor. **Even mathematical proofs are not timeless.** They temporarily satisfy their reviewers in a conversation.

Camerer has written an article which appeals to a social, nonepistemological standard of persuasion by the very act of trying to persuade the strategic management audience that mere persuasion is not enough. I also note that Camerer uses the "unscientific" metaphor of the tortoise and hare to argue for the choice of deduction over induction as the best way forward in strategic management. My point is not to criticize Camerer and recommend that he attempt the impossible by avoiding metaphor. I submit that Camerer's writing style is charming and that he should not feel uncomfortable by possessing these glorious skills of our humanity as "awkward" as they may be.

Finally, I come back again to the pragmatic voice of John Dewey (1929) who suggested that we eliminate the distinction between art, science and philosophy. Also the "poetic" genius Einstein found the distinction between art and science absurd. He eloquently stated the commonality of art and science in the following way:

> One of the strongest motives that lead men to art and science is escape from everyday life ... A finely tempered nature longs to escape from personal life into the world of objective perception and thought. ...Man tries to make for

> himself in the fashion that suits him best a simplified and intelligible picture of the world; he then tries to some extent to substitute this cosmos of his for the world of experience, and thus to overcome it. This is what the painter, the poet, the speculative philosopher and the scientist do, each in his own fashion (1934, p.2).

4. Induction versus Deduction. Camerer's 4th major concern involves the induction-deduction puzzle. I regard the inductive-deductive debate as a pseudo-problem; it cannot be solved even within the context of the framework in which it is posed. Induction/deduction is not a useful dichotomy. Induction and deduction are inextricably intertwined (Hunt, 1983; Wallace, 1969). Does Camerer use inductive or deductive reasoning in making his case for deductive reasoning?

Even granting Camerer's premise that the induction/deduction dichotomy is useful as a demarcation criterion, my position on this matter was probably best put by the English economic historian T. S. Ashton (1971, p. 177):

> The whole discussion as to whether deduction or induction is the proper method to use in the social sciences is, of course, juvenile: it is as though we were to debate whether it were better to hop on the right foot or on the left. Sensible men with two feet know they are likely to make better progress if they walk on both.

A strategic management professor hopping along without an inductive leg, unless he or she is a decathlon athlete, will have a narrow perspective and little ability to apply strategy to complex issues. Ashton's metaphor may be applied to the process/content debate in strategy as well (Mahoney, 1990).

STRATEGY CONTENT AND THE RESEARCH PROCESS: A REPLY TO MONTGOMERY, WERNERFELT AND BALAKRISHNAN

Montgomery, Wernerfelt, and Balakrishnan (M-W-B, 1989) provide a provocative methodological paper for the strategic management audience to encourage soul-searching on the research process. They also explicitly encourage others that see things differently to present their views. I take up their invitation.

M-W-B begin their argument by suggesting that: "research progress is a continuous expansion of knowledge involving the generation, refutation, and application of theories" (1989, p. 189). I would like to raise some doubts about whether it is "continuous" and suggest that "progress" is a problematic concept.

Kuhn (1970, 1977) provides a persuasive argument that science does not progress in a continuous fashion. Kuhn suggests that science develops in a discontinuous manner and that historical misconstructions render scientific revolutions invisible. Kuhn suggests that: "the member of a mature scientific community is, like the typical character of Orwell's 1984, the victim of a history rewritten by the powers that be" (1970, p. 167).

Of course, once we question the continuity argument, we may also begin to question what is meant by "progress". Do we achieve progress by continuing "normal science" or by choosing a new paradigm or perhaps by adopting multiple paradigms? I believe that theoretical pluralism (Boland, 1982; Bowman, 1990; Caldwell, 1982) is the most cogent argument and that empirical content is enhanced in the process.

Kuhn's thesis makes me skeptical that adopting a central paradigm is the enlightened view. Kuhn argues that it is rather presumptuous to believe that the adoption of a new paradigm is any closer to the "truth" than the older paradigm. Kuhn maintains that: "There are losses as well as gains in scientific revolutions, and scientists tend to be peculiarly blind to the former" (1970, p. 167).

I also believe that M-W-B's five propositions (1989, pp. 190-191) require closer scrutiny:

1. All theory generation should depend on some past observation.
2. All observations should be guided and interpreted through some theory.
3. A theory is better, ceteris paribus, (a) if it is refutable and (b) if it is consistent with a body of existing theories.
4. A good test is one that can refute an explicit theory.
5. The sciences should be undertaken for the sake of ultimate application.

I would argue that the first two propositions do not require the word "should". Thus I maintain that: (1) All theory generation **necessarily** depends on some past observations and that (2) All observations are **necessarily** guided and interpreted through some theory.

The first proposition is one that Einstein (1934) repeatedly emphasized. Science **must** start with facts (observations) and end with facts (observations). This pragmatic proposition is sometimes called "epistemic empiricism" (Kaplan, 1964). We cannot know without depending on experience.

The second proposition suggests that "believing is seeing". As my conversation with Camerer suggested: "all facts (observations) are theory-laden". There can be no immaculate perception. As Hanson noted: "There is more to seeing than meets the eyeball" (1965, p.7).

With the distinction that I would replace the "ought" statements by "is" statements in propositions 1 and 2, I am in agreement with the spirit of M-W-B's argument. However, in proposition 3, I challenge both parts (a) and (b).

I have already articulated my skepticism with Popper's falsifiability thesis in my discussion with Camerer. I simply make the additional point that if M-W-B take their first two propositions seriously then there can be no theory-neutral observational facts to refute a theory. This of course, is the crux of Kuhn's attack on Popper's falsification thesis. I suggest that M-W-B's first three propositions provide an interesting paradox for the strategic management audience: How can we demand the vigorous testing of theories in terms of their observable predictions, while at the same time granting that all observations are theory-laden?

I am persuaded that since all facts are theory-laden, if we want more facts, then we need more theories. This is Feyerabend's (1975) "principle of proliferation". If different "conceptual lenses" (Allison, 1971) magnify, highlight and reveal as well as blur and neglect salient "realities" then why must we choose between alternative models? Students that go about trying to falsify two of Allison's three models to explain the Cuban Missile Crisis have really missed the whole point of what Allison's book was trying to communicate.

In the strategy literature one of the common questions of the day is the "so what" question. What difference does your view make? The answer I submit is the following: I wholeheartedly believe that Allison provides us with a fundamental historical lesson. In a nuclear age, it is critical that we train people to utilize "multilectic inquiry" (Huff, 1981). Theoretical pluralism, tolerance and understanding makes "groupthink" less likely in our leaders, in our organizations, and in ourselves (academe).

Having articulated some of the views of Myrdal above, it seems only fitting that I explicate the views of the co-winner of the 1974 Nobel prize in economics, Friedrich Hayek. Hayek argued that it was no exaggeration that once the more active part of the intellectual community has been converted to a set of beliefs, the process by which these become generally accepted is almost automatic and irreversible. He argued that the process of opinion forming by intellectuals depends on freedom of thought and expression. The ideal of democracy rests on the belief that the view which will direct government emerges from an independent and spontaneous process. The best intellectual design comes about by the free competition of individuals, not by coalitions or collectives that plan on espousing homogenous half-truths. A "manifesto" that delineates a methodological plan for us all to follow is ill advised. If the academic community attempts to consciously control the intellectual process then we may well be on the road to serfdom (Hayek, 1944).

I also have some reservations that a theory is better if it is consistent with a body of existing theories. I am in accord with M-W-B as a personal faith but I (as a reviewer of a paper) would not hold others to this standard. I think it is legitimate to ask: Why should we demand consistency? Kaplan (1964, pp. 314-315) expressed this idea eloquently:

> Coherence is a conservative principle which ruthlessly suppresses as rebellion any movement of thought which might make for a scientific revolution. The unyielding insistence that every new theory must fit those theories already established is characteristic of closed systems of thought, not of science.

In proposition 4, M-W-B suggest that a good test is one that can refute an explicit theory. I argue that no such test exists. M-W-B suggest that a theory that proposes that:

$$X_1 = X_2^a \ln X_3 + b X_4 \quad , \; b>0$$

"stands or falls on the result of a single test" (1989, p. 191). The view that a theory can be refuted by a single test is referred to by Blaug as "naive falsificationism" (1980, pp. 26-27). Popper, whom M-W-B cite approvingly, while being accused of being a "naive falsificationist", has advocated Duhem's irrefutability thesis (1965, p.50):

> In point of fact, no conclusive disproof of a theory can ever be produced, for it is always possible to say that the

> experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental result and the theory are only apparent and that they will disappear with the advance of our understanding.

In my search for understanding, I have reached the following tentative conclusion: Not only is affirmation impossible, which is the consensus of most social scientists, but refutation is also impossible. This realization opens my mind to be more tolerant rather than leading me to despair. In a world of chance, scientists must take their chances. While affirmation may be impossible, we affirm that we shall meet in San Francisco for the Academy of Management meetings in August, 1990. While refutation may be impossible, tests do influence our Bayesian priors and perhaps for the better.

This conclusion that I reached, I have since found out is not so "new". Richard Lipsey in his second edition of the popular text: An Introduction to Positive Economics, long ago reached the conclusion that I now hold. Lipsey argued that (1966, p. xx):

> I have abandoned the Popperian notion of refutation and have gone over to the statistical view of testing that accepts that neither refutation nor confirmation can ever be final, and that all we can hope to do is discover on the basis of finite amounts of imperfect knowledge what is the balance of probabilities among existing hypotheses.

M-W-B in their fifth proposition assert that "the sciences should be undertaken for the sake of ultimate application" (1989, p. 191). I concur. The pragmatist suggests that the truth or validity of an idea depends upon the consequences of acting upon it. Pragmatism explicitly makes action the primary context of all meaning and value (Dewey, 1929). This is pragmatism's conception of truth in strategic management.

While being in almost total agreement with Camerer and M-W-B on the articles that they cite as exemplary work that illustrate the health and vitality of strategic management, I fundamentally disagree with their "worldview". I suggest two rudimentary factors that lead to my dissent.

First, I lean more toward the **rationalist** persuasion while M-W-B, and particularly Camerer, have an **empiricist** orientation (Bowman, 1990). Bowman (1990) defines the rationalist persuasion as viewing the mind as actively organizing experiences on the basis of pre-existing schemes. The empiricist, on the other hand, treats mental processes as a reflection of information obtained from the environment. Or as Rorty (1979) puts it, the empiricist holds the Lockean metaphor of the "mind as a mirror of nature". I see M-W-B recognizing the rationalist perspective in their early propositions but then pulling back to the empiricist view.

I hold the view that scientific statements are not true or false of some external, independently existing "reality" but rather are creations or constructions of the human mind. I am sensitive now to the notion that agreed facts are theory-laden making the choice between theories problematic and I deny the existence of rational, universally valid criteria for the evaluation of scientific inquiry. "Truth" is a fifth wheel. The question what is "truth" is replaced by: "How do we come to endow experience with meaning?" (Bruner, 1986).

To paraphrase Herbert Simon (1982, p. 441), strategic management is one of the sciences of the artificial. Scientific propositions are artificial creations. To maintain that scientific observations are descriptions of the world based on the generalization of experiments is the "myth of the scientist". Dewey (1929) suggested that "truth" be replaced by "warranted assertability" and that there are no assertions immune from revision.

Second, I believe that Camerer and M-W-B have been strongly influenced by the writings of the <u>young</u> Karl Popper. M-W-B cite two of Popper's works and Camerer, while not citing Popper directly, does cite two of Popper's most ardent followers in economics (Blaug, 1980; Friedman, 1953). M-W-B and especially Camerer, advocate Popper's methodological principles. I would suggest however that reference to philosophical authority on these matters is a tactical error. Many philosophers such as Polanyi, Hanson, Toulmin, Kuhn, Feyerabend, Rorty, Kaplan, and even the older Popper (1970) have raised serious questions about logical positivism (Ayer, 1959) and reductionism.

My message then is not comforting to those who prefer that methodology offer a rigorous, objective, prescriptive framework. In fact I am offering a nonprescription. Criteria of elegance, multiple connectedness, intuitive plausibility, simplicity, predictive power, generality, realism, and so forth are based on metaphysical assumptions. For example, the attempt to define precisely what is meant by "simple" have failed (Hempel, 1966, pp. 40-45). Are models of perfect rationality more "simple" than models of bounded rationality? Herbert Simon (1982, p. 476) in his Nobel prize acceptance speech noted that Occam's razor (accept the simplest theory that works) has a double edge. Succinctness of statement is not the only measure of a theory's simplicity.

I am not a "relativist" claiming that two incompatible statements are equally good but rather I am submitting that the grounds for choosing between such opinions are less algorithmic than has been claimed. In the same way that Bourgeois, Bowman, Huff and others argue for theoretical and methodological pluralism, I am arguing for pluralism in "how we explain". The principle of the autonomy of inquiry should not be compromised. I envisage the strategic management community as a paradigm of the open society. In my view the virtue of Rorty's philosophy, following Dewey (1929), is that it exemplifies a nonepistemological philosophy in which good science is good conversation (McCloskey, 1985).

The quest for certainty is not the quest for wisdom (Dewey, 1929). Wisdom consists in the ability to sustain a conversation. Conversational justification is naturally holistic, whereas the notion of justification embedded in the epistemological tradition is reductive and atomistic (Rorty, 1982). To use a popular metaphor, the "rules of the game" are conversational norms: don't lie; pay attention; cooperate; don't shout; let other people talk;

be open-minded; explain yourself when asked; don't resort to violence or conspiracy in aid of your ideas (McCloskey, 1985).

To conclude this essay in persuasion, I would like to provide the reader with Oakeshott's observations on "the voice of poetry in the conversation of mankind". A message that I would apply to scientific dialogue as well (Oakeshott, 1962, pp. 198-202):

> In a conversation the participants are not engaged in an inquiry or a debate; there is no "truth" to be discovered no proposition to be proved, no conclusion sought ... Nobody asks where they have come from or on what authority they are present ... Their is no symposiarch or arbiter, not even a doorkeeper to examine credentials. Every entrant is taken at face-value and everything is permitted which can get itself accepted into the flow of speculation. And voices which speak in conversation do not compose a hierarchy. Conversation is not an enterprise designed to yield an extrinsic profit, a contest where a winner gets a prize, nor is it an activity of exegesis; it is an unrehearsed intellectual adventure. ... As civilized human beings, we are the inheritors, neither of an inquiry about ourselves and the world, nor of an accumulating body of information, but of a conversation, begun in the primeval forests and extended and made more articulate in the course of centuries. It is a conversation which goes on both in public and within ourselves. ... Education, properly speaking, is an invitation into the skill and partnership of this conversation in which we learn to recognize the voices, to distinguish the proper occasions of utterance, and in which we acquire the intellectual and moral habits appropriate to conversation ... (I)n its participation in the conversation each voice learns to be playful, learns to understand itself conversationally and to recognize itself as a voice among voices.

To the eloquent conversation provided by Bourgeois, Bowman, Huff, McCloskey, Oakeshott, Rorty and others, I would like to add an additional comment, if I may. Strategy research should concern itself with continuing the conversation of the field rather than insisting upon a place for universal methodological criteria within that conversation. If as members of the strategic management community we do not joyfully discuss our ideas and if we are excessively concerned about the reaction of the "professionals" then we are abandoning hope for an open pluralistic dialogue and we will have only ourselves to blame. The end result would be not only poor conversation but also poor science.

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ACKNOWLEDGEMENTS

The author wishes to thank Edward H. Bowman, Irene Duhaime, Anne Huff, and Howard Thomas for helpful comments on an earlier draft of the paper. An earlier version of the paper was presented at a conference on "Theory Building in Strategic Management" held at the University of Illinois -- Urbana in May, 1990.